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***Interactive comment on* “Emulation of a complex global aerosol model to quantify sensitivity to uncertain parameters” by L. A. Lee et al.**

Anonymous Referee #2

Received and published: 12 September 2011

This paper proposes a computationally efficient method to quantify the sensitivity of a global aerosol model to a set of 8 parameters, including interaction effects between the parameters. Although the statistical approach has been used for other types of computer models, the application to the global aerosol model is novel. As such, the authors do a good job providing background, motivating examples and a fairly detailed literature review of research related to this topic. They are also very clear about the underlying assumptions and limitations of the analysis which is appreciated. The manuscript is organized and well written, and the subject matter is appropriate for ACP. I recommend that it be published after minor revisions. I have a several concerns/questions that I think would be helpful for the authors to respond to (note this does not necessarily mean changing the underlying analysis for this publication) followed by a handful of minor edits/suggestions.

C8874

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In terms of the types of conclusions this paper attempts to make (e.g. ranking what parameters have the largest impact on the model predicted CCN and the role of interaction effects), the analysis seems quite limited by the capabilities of the software being applied. A monthly averaged model value (from a single month) from two grid cells does not seem sufficient for prioritizing research for a global model of complex aerosol processes (one of the ultimate goals for research of this type that the authors mention in the abstract). It would be very valuable if the software could be extended to allow for multivariate emulation of the function output as suggested by J. Rougier's ACPD comment in order to build confidence in robustness of the stated conclusions. The issue of calibration through comparison to observations also seems extremely important. The authors state it is possible to use calibration against observations to remove implausible regions of input space, but in practice this would seem a rather daunting task due to limitations in available observations and the complexity of the modeling system. I wonder if the authors could comment on how difficult these types of extensions really are. Are these very reasonable next steps or is this something that is a long way from being possible for a computer model of this scale and complexity?

My second question is the justification given that the errors in emulator are sufficiently small and can be completely ignored in the sensitivity analysis. On pages 20451 and 20452 you provide the emulator standard deviation for the CCN concentration at the surface for London (2.1 cm^3) and the remote marine site ($.5 \text{ cm}^3$). It is true that these values are quite small compared to the estimated uncertainty due to the input ranges of the 8 sensitivity parameters. However I wonder if this is the most appropriate comparison. Figure 5 shows the 95% confidence intervals for these sites compared to the validation runs. Many of these intervals appear to have standard deviations on the order of 20 cm^3 for the London cell and 5 cm^3 for the remote cell (estimating one quarter the length of the 95% interval). Since you use the emulator to make predictions under these different parameter settings, not just for the base-line conditions, it would seem the error represented by the spread of these confidence intervals is very relevant to the interpretation of your sensitivity analysis. It would be helpful if this issue could

C8875

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be discussed and justified further in the text (or clarified, if I have simply misinterpreted these figures and the conclusions on these pages).

One general editorial comment: There are numerous missing commas throughout the entire manuscript (too many to enumerate here). I recommend a technical edit before final submission.

A few specific comments for each section:

Abstract:

– line 3: You may wish to better clarify/elaborate on what you mean when you say this analysis is necessary “to help understand model diversity”.

– line 26: “. . .models.”

Introduction:

– pg 20435, line 28: “..develop further in the future.”

Section 2:

– pg 20442, line 15: Recommend removing the word “too”.

Section 4:

– pg 20452, line 29 – pg 20453: Long run-on sentence. Perhaps end sentence after “. . .effect is 126%.” Also recommend inserting “. . .shown by the fact that the total. . .”.

– pg 20453, line 18-23: Sentence beginning “It is thought the increased. . .” could use some wordsmithing. Also the following sentence beginning “At the higher. . .” is rather awkward and should be reworded.

– pg 20454, line 24: Replace “improve” with “reduce”.

– pg 20454, line 27: “. . .vertical profile”

Section 5:

– pg 20457, line 13-14: Suggest removing one (or both) “simply”.

Figures:

Figure 3 caption: “. . .the mean can not be considered. . .”

Figure 9 caption: “. . .is shown to differ. . .” Also suggest changing the last sentence “. . .high oxidation diameter, as indicated by the spread. . .”

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20433, 2011.

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